REVIEWS.

ART. XIII.—*Anæsthesia.*

1. *Chloroform: its Action and Administration. A Handbook.* By ARTHUR ERNEST SANSOM, M. B. Lond. London, 1865. pp. 192.
2. *On the Safe Abolition of Pain in Labour and Surgical Operations by Anæsthesia with Mixed Vapours.* By ROBERT ELLIS, Surgeon-Accoucheur. London, 1866. pp. 80.
3. *On Stimulants and Narcotics: with Special Researches on the Action of Alcohol, Ether, and Chloroform on the Vital Organism.* By FRANCIS E. ANSTIE, M. D., M. R. C. P., Assistant Physician to Westminster Hospital, Lecturer on Materia Medica and Therapeutics, etc. London, 1864. pp. 488.
4. *A System of Surgery.* Edited by T. HOLMES, M. A. Cantab. Vol. III., Art. "Anæsthetics," by JOSEPH LISTER, Esq., Professor of Surgery in the University of Glasgow. London, 1862.
5. *Traité d'Anesthésie Chirurgicale.* Par MAURICE PERRIN, Professeur agrégé à l'Ecole impériale de Médecine et de Pharmacie militaires, Lauréat de l'Institut, Chevalier de la Légion d'Honneur, etc. etc. etc., et LUDGER LALLEMAND, Professeur agrégé à l'Ecole impériale de Médecine et de Pharmacie militaires, Lauréat de l'Institut, Chevalier de la Légion d'Honneur, etc. etc. etc. Paris, 1863. 8vo. pp. 668.
   A Treatise on Surgical Anæsthesia. By MAURICE PERRIN and LUDGER LALLEMAND.
6. *Nouveau Dictionnaire de Médecine et de Chirurgie pratiques.* Tome deuxième. Art. "Anesthétiques généraux," par J. GIRALDES. Paris, 1865.
   New Dictionary of Practical Medicine and Surgery. Art. "General Anæsthetics," by J. GIRALDES.
7. *Das Chloroform. Eine Zusammenstellung der bisher über dasselbe gemachten wichtigsten Erfahrungen und Beobachtungen, vorzüglich in physiologischer und medizinischer Beziehung.* Von Dr. FRIEDRICH SABARTH. Würzburg, 1866. 8vo. pp. 276.
   Chloroform. A Collection of the most important Experience and Observations made up to the Present Time. By Dr. FRIEDRICH SABARTH.

TWENTY years have elapsed since the discovery of anæsthesia brought into the world of reality what had before existed only in the fancy of the poet—a "borrowed likeness of shrunk death," producible at will, and of temporary duration. How unexpected was the achievement may be known from the fact that, only a few years before it was made, the leading surgeon of the world spoke of it as a chimera the pursuit of which was abandoned.[1] Yet, in fact, like many other discoveries in science and in art, it was the result of long-continued and patient efforts made by many persons, and not accidental, but the termination of a pursuit carried on for years, with the definite end in view which was afterwards attained. The discovery found, too, a profession, if not a world, ready to receive it; its spread was as rapid as the transmission of news; and it may truthfully be said that the patient, whose agonies it annihilated, did not give it a heartier welcome or receive it with a more profound thankfulness than did the surgeon, who saw his feelings of humanity spared, while his operations were aided and the field of his usefulness extended.

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[1] "Eviter la douleur dans les operations est une chimère qu'il n'est pas permis de poursuivre aujourd hui."—VELPEAU, 1835; from KIDD's *Manual of Anæsthetics*, London, 1859.

The twenty years since the discovery have not passed without progress. The first success was not final; another and more powerful agent was soon discovered, and has almost entirely supplanted the one first used; several others have been introduced, experimented with, or used for a time, and abandoned. More powerful than all else to stimulate inquiry has been the shadow which soon came to darken the lustre of a discovery which, it would seem, might otherwise have been too perfect for our conditions here. Death was found sometimes to occur during the process of anæsthesia—sudden and unexpected death. The patient who sought relief from temporary pain found the termination of existence, and the surgeon was horrified at seeing his attempt to alleviate end in destruction. This sad fact has been the cause of more experiment and investigation than all else connected with the subject, and although certainty may not yet have been attained—although opinions have differed widely, and still differ, as to the circumstances which cause or favour this sad occurrence—the results have been a better acquaintance with the agents employed, clearer views of the sources of danger, and more certain rules for safety in their administration.

The work of Snow, which appeared in 1858, seemed to mark a period in the literature of anæsthesia. His views were so ably presented and supported by so much clinical experience, and his work was so complete a treatise upon the subject, that the profession generally accepted him as authority, and for some years his work and doctrines were sole occupiers of the field. But the brief space of time since then marks a wide interval of progress and research; our knowledge has been much increased; opinions have changed; and within a few years many works have issued from the press to give expression to them. The titles of some of these we place at the head of this article, and will briefly indicate their aim, scope, and general character, while we will try to gather from their pages the latest information and give the most reliable decisions upon some of the more important questions connected with anæsthesia and anæsthetics.

The work of Dr. Sansom may be characterized as most excellent. Written not alone from a theoretical point of view, but showing very considerable experimental study and an intimate clinical acquaintance with the administration of these remedies—passing concisely over the whole ground, giving the latest information upon every point—it is just the work for the student and practitioner. The author may rest assured that although in his preface he objects to the "hackneyed expression of 'endeavouring to supply a want,'" this is just what he has done—supplied, and well supplied, a want, for no such book existed before in our language. That he has "expended much labour in its compilation" is evident throughout, and we are certain that he has succeeded in his endeavours "to combine the characteristics of truth and brevity."

The work of MM. Perrin and Lallemand is a complete and exhaustive treatise upon the subject. With less of personal clinical experience, perhaps, than might have been wished, the work contains chapters upon every subject connected with anæsthesia, from a thorough history of the discovery, with a critical examination of the merits of rival claimants for the honour, to a full consideration of what has only so recently attained practical realization—local anæsthesia. The chapters upon the physiological action of ether and chloroform are especially full; there is a complete list and full report of all the sudden deaths during administration of either agent which had been reported up to the time of publication; every theory advanced upon the nature or cause of this sad accident is given and thoroughly examined; finally, the application of the remedies to all the various operations of surgery and to obstetrics is fully considered.

It would seem scarcely possible that in these days of international communication such a work as this, upon such a subject, could escape observation and comment during several years. Yet such is the fact. We do not remember to have seen even a notice of this work in any medical journal, either foreign or domestic. Nor is this all; in all the works upon chloroform we have thus far perused we have not found an allusion to, nor an extract from, the book. What makes this more passing strange is the fact that these authors, with an associate, Duroy, have published a work[1] devoted to an exhaustive examination of the physiological action of these and similar remedies, and the doctrines advanced in which have given rise to no small amount of controversy among writers upon physiology and anæsthetics, and it is therefore referred to and quoted from very frequently in every work upon these subjects! So singular a fact we cannot fail to mention, while to explain it is impossible.

The present work bears upon its title-page the names of Perrin and Lallemand. A fly-leaf follows, and informs the reader that during the preparation of the volume M. Lallemand was called away to the post of "médecin en chef" of the French expedition to Mexico; that but a short time elapsed before "a glorious death came to change the sorrows of absence into eternal regrets." The publication would then have been abandoned, "had not the presentation of common researches and of ideas which had been cultivated together" appeared to his colleague "as a sacred debt which was owed to his memory, as a pious homage to be rendered to his friendship."

Dr. Sabarth's work follows very closely the design expressed in the title-page; it is a collection of the writings of others, rather than an original work. As affording a ready reference to the views of those who have written upon the subject, and especially as gathered from medical journals often inaccessible, it is valuable; but it lacks the impress of individual opinion, and a running commentary upon opinions advanced, often conflicting, cannot possibly assume the character of a scientific treatise. It passes very completely over the whole subject, and shows evidence of great industry in the collection of material from widely different sources.

The little volume of Dr. Ellis has been written with two objects in view: the demonstration of the practical utility of mixed vapours for anæsthetic purposes, and the introduction to the profession of a special inhaler, designed by himself, for their administration. It is an admirable little work, from the scientific spirit which animates it, and the judicious and moderate tone of expression which pervades it. He has worked long and faithfully at the subject by actual experiment, especially to learn the different rate of evaporation of alcohol, ether, and chloroform when combined, and his inhaler is a very ingenious piece of mechanism. The instrument and his mode of anæsthesia he claims to be particularly adapted for natural labour, for which they were designed. Upon one point the author deserves the greatest praise: having a special plan and instrument to present, he has not sought to build up a reputation for them by pulling down that of others. Regretting the accidents which sometimes follow the use of chloroform, he acknowledges they are very few indeed; and firmly believing in the superior safety of the mixed vapours, he does not exaggerate the danger of the pure article, or try to bring it into disrepute. In this respect he escapes a grave mistake made by Dr. Arnott in his advocacy of local anæsthesia by cold.

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[1] Du Rôle de l'Alcool et des Anæsthétiques dans l'Organisme. Recherches expérimentales. Par Ludger Lallemand, Maurice Perrin, et J. L. P. Duroy. Paris, 1860.

Dr. Anstie's work, so far as his doctrines in regard to stimulants are concerned, was fully reviewed in a late number of this journal. Only about one hundred pages of the volume are taken up with ether and chloroform, and this chiefly in regard to their physiological action. He is an original worker and independent thinker; his opinions and conclusions are valuable, and cannot be neglected in any view of the present condition of the subject.

Of the articles in the *System of Surgery* and the *Dictionnaire* we need only say they are worthy of a place in what may be called these national representative works. In both the subject of anæsthesia is briefly but most ably considered, and with direct reference to the demands of every-day practice.

We shall occupy but brief space with the history of the discovery of anæsthesia and the agents by which it is produced. One or two points demand brief mention. Thus, the French claim for Flourens priority of discovery of the anæsthetic properties of chloroform upon animals, an honour which is universally accorded to Simpson by English writers, although Sansom speaks of "certain observations" on this point communicated by the celebrated physiologist to the Academy of Sciences of Paris, in March, 1847.[1] At any rate, Flourens' observations bore no practical fruit, and are but another of those instances which excite our wonder at the near approach made to great results without attaining them. To render him justice for his discovery, however, is not to detract from the honour due to Simpson, whose personal daring in experimentation with the vapour, and his merits in verifying the efficacy and safety of its use, are beyond all praise. Rich and lasting as time, too, will be his reward. Often has it been impressed upon us as we have heard in some lonely cabin in the forests of a distant western State whose inmates never heard his name, a hearty "God bless the man who invented chloroform" coming from a heart overflowing with thankfulness for suffering abolished.

Upon one point, and one only, are we obliged to find fault with Dr. Sansom, and it is in connection with the history of the discovery. It is the entire omission of any mention of the first surgical operations in this country under ether.

"A man named Frost had a tooth extracted under the influence of sulphuric ether, absolutely without his knowledge of the operation, on the 13th of September, 1846.

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[1] The reference in Perrin's work is to *Comptes Rendus de l'Académie des Sciences*, 1847, tom. xxiv. p. 342.

> "The boon was too great not to be recognized. The news was brought to England, and ether was immediately employed for surgical operations by Mr. Liston and Mr. Fergusson."

This is all! Not a word of the operations at the Massachusetts General Hospital by Warren and Hayward! Surely this was not ignorance on the part of the author, but could only have arisen from carelessness. Certainly, for the honour of our country, we could not let it pass unnoticed.

The decision of foreign writers as to the controversy between Jackson and Morton for the honour of the discovery cannot but be of sufficient interest to deserve a place here. Giraldes briefly alludes to the long controversy before Congress and the Medical Society of Boston, into the examination of which he cannot enter, and says "the honour was accorded to Dr. Morton." The following is Perrin's decision, which seems to be assented to by Sansom:—

> "Jackson first placed beyond doubt this capital fact, that general insensibility is one of the constant effects of ether upon the organism; he recognized besides that this insensibility could be obtained in a very short time, that it disappeared rapidly, and that it was not accompanied by the dangers which had been attributed to it. But these fundamental discoveries, rather obscurely seen than demonstrated by experience, needed fructification by an agent of execution docile and enterprising, and venturesome in proportion to the little he knew of the question.
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> "To Jackson belongs the credit of the idea; to Morton the realization of the idea: without the former etherization would not have existed; without the latter its advent would have been retarded many years. Both, by different claims, merit much from the human race for endowing surgery with a heroic means of abolishing physical pain. But if posterity erects statues to Jackson, she will hesitate perhaps to recognize as one of her benefactors a vulgar merchant of letheon." (p. 61.)

"A vulgar merchant of letheon!" the whole course of this individual since M. Perrin's work was published, but confirms the justice of the sentence. What was true then, is true now, and "pity 'tis 'tis true."

> "Morton, from the first day, had seen in the assured power of abolishing pain only a good business to follow; thus, his first care was to monopolize, for his own profit, the benefits of etherization." (p. 57.)

For the honour of our country we deeply regret the facts in regard to this discovery, but can still congratulate ourselves that our profession is not implicated in the mercenary traffic which was attempted.

In tracing the early part of the discovery of anæsthesia M. Perrin seems to have followed Figuier (*Découvertes Scientifiques; l'Etherization*, t. iii.), for he quotes him frequently. In the later part he extracts freely from Jackson's "Defence," which is, of course, the testimony of an interested party. Certainly we know of very well informed medical men of this country who doubt whether M. Perrin could substantiate what he says as to Jackson's share in this discovery, especially in regard to his having placed beyond doubt that anæsthesia by ether was free from danger.

But there is another man belonging to this country, too, whose share in the discovery of anæsthesia has never been recognized as fully as it undoubtedly deserves to be. We allude to Horace Wells, of Hartford. He, either independently, or acting upon the long-forgotten suggestion of Sir Humphrey Davy, that nitrous oxide might be used to abolish the pain of surgical operations, pursued the study of anæsthesia by this gas with ardor and perseverance, and there is little room for doubt that he first established the fact of anæsthesia, and glad we are to see that M. Perrin gives him full credit for it.

> "The honour of the first public and authentic trial of surgical anæsthesia, by the aid of means newly discovered, belongs entirely to an obscure dentist of Hartford, a small town of the county of Connecticut."[1] (p. 47.)

This successful trial occurred several years before Jackson proposed, or Morton used ether; both these men had opportunity of communicating with Wells, and they knew the direction of his labours, and had witnessed some of the results, and there is no doubt that much for which they have obtained credit was derived from him—that the modern source and origin of anæsthesia was the brain of Horace Wells.

Yet his memory is neglected, and his name almost forgotten. Poor Wells! in all the annals of science we know no history more sad than his. Seeing clearly the good to be attained, yet failing to realize it, disappointed, chagrined, driven from the amphitheatre by the derision of a pack of thoughtless students, his heart broke under the trials, and with his own hand he terminated a life which had been spent in endeavouring to confer upon the race the greatest boon to suffering humanity. He died with the discovery almost within his grasp. He was a Columbus, whose bark went down within sight of the new land.

But, into this question as to the real discoverer of anæsthesia we cannot go. An impartial history of the subject has not yet been written, and probably will not be until all the actors have passed from the stage; then the relative claims of Wells, Jackson, and Morton may be impartially examined, and the merit of each impartially determined.

Authors are not agreed as to the mode of action of ether and chloroform. Leaving out of view the more mechanical theories—such, for instance, as that of Faure, which based anæsthesia upon a local change in the respiratory tissues—and which have been conclusively shown to be untenable, there remain two leading doctrines as to the *modus operandi* of these agents. One of them is that anæsthesia is produced by an interference with the interchange of oxygen and carbonic acid in the blood, and is therefore asphyxia; the other, that the agents circulate in the blood, and have an especial elective affinity for, and exert a direct action upon, the nerve tissue; that, acting as a direct poison, they abolish consciousness and the sensomotor power. The latter doctrine is maintained by Perrin, and the experimental evidence which seems to substantiate it is more fully given in the physiological work on the *rôle* these agents play in the economy, which we have already mentioned. Of the former theory Dr. Sansom is the especial advocate. It will be seen that it is entirely *physico-chemical;* to those who maintain it *vital* actions are nothing.

> "Anæsthetics are agents which, when absorbed into the circulation, exert an influence upon the blood. They are shown to have the power of altering its *physical character* and its *physical properties.* * * * Narcosis ensues; and is due, not to the influence of a circulating poison, but to the influence of an altered blood." (*Sansom*, p. 55.)
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> "Whatever produces, to a certain extent, insufficient aeration of the blood, produces narcosis; and whatever produces narcosis, produces, by some means or other, imperfect aeration of the blood." (p. 62.)

Of this doctrine we have to say, first of all, that as it is physico-chemical in nature, so it is based too exclusively upon physico-chemical experiments. We refuse to accept, as having more than a remote bearing upon the decision of the question, such evidence as that presented on page 43; because Gosselin "exposed the brain of a dog and poured chloroform thereupon," and because he "divided the organ and applied chloroform," and "no diminution of sensibility followed," we do not believe it therefore follows that anæsthesia is not produced by direct action of the remedy as it circulates through the nervous centres after inhalation. Neither do we accept as proof the fact that when chloroform is added to animal or human blood, it changes the shape of the corpuscles, and destroys them. Referring to Dr. Harley's experiments,[1] upon which the author lays great stress, we find them of the same nature; chloroform added to blood out of the body, interferes with the taking up of oxygen, and promotes the absorption of carbonic acid. To add a powerful chemical body to drawn blood, and attempt to deduce conclusions, from its coagulating or chemical action, as to its physiological action when introduced into the vital current by inhalation, is, it seems to us, to violate all sound rules of experiment and deduction; it is confounding the physico-chemical action of the agent with the subtle changes it occasions in the production or manifestation of nerve-force.

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[1] We translate as nearly literally as possible.

Again, we suggest that to term the anæsthetic process asphyxia is to necessitate a special kind of asphyxia to meet the case, for it certainly does not resemble the ordinary kind. In asphyxia, sensation is only lost under the influence of the circulation of black blood; in anæsthesia the blood does not become black, or nothing like of the deep hue of asphyxia, when sensation is abolished, unless the inhalation of the anæsthetic be *prolonged*, or in the cases of experiment on animals which are made to breathe the vapours from closed receptacles, thereby complicating the influence of the remedy with that of expired air; in asphyxia the loss of sensation is a secondary phenomenon, in anæsthesia it is primary; "numerous researches made by Renault, Longet, Blandin, and Guerin do not permit us to doubt that in anæsthesia complete loss of sensibility occurs before the arterial blood changes its colour."[2] The motive power, too, is differently affected in the two processes; anæsthetics abolish it, while on the contrary it persists in asphyxia to the end; "by mechanical irritation convulsions can be caused up to the moment of death."

Probably the weakness of M. Perrin's argument is in the fact that he bases it too exclusively upon the *quantity* of chloroform formed in the different tissues after death. He and his colleagues determined this to be as follows: "The blood being taken as the standard representing 1, the brain contained 3.92 parts; the liver, 2.08; the muscles, 0.16;" this view, therefore, is that "anæsthetics accumulate in the nervous centres by virtue of an elective affinity." Upon this point Dr. Sansom attacks them, and especially in regard to the large amount shown to be also in the liver. But it appears to us that the quantity thus found after death, is not necessary to the argument, and that the doctrine might be maintained had the amount never been determined. Nothing is better established in therapeutics than that certain medicines act invariably upon certain organs however introduced; that some pass out by one channel and some by another; we may call it "elective affinity," or what we will, the facts remain the same, and we should prefer to base the theory of the action of these remedies rather upon reason, the results of analogy, of clinical observation, and physiological experiment, than upon physics, chemistry, or post-mortem examinations. Viewing the question by these lights, we feel compelled to adhere to the vital theory until we see something more conclusive than has yet met our eye.

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[1] British and Foreign Medico-Chirurgical Review, Oct. 1856, p. 429.

[2] Perrin, p. 191.

> "What experimental physiology permits us to establish is confirmed by clinical experience. During operations performed upon man in a state of complete anæsthesia, nothing calls to mind the idea of asphyxia; in the latter the mental faculties are simultaneously affected, the skin is marbled, the tissues turgid, the arterial blood of deep black hue; in the former, the mental faculties are only progressively invaded, the face is pale, cadaverous, and the blood escapes from the arteries in vermilion jets." (*Perrin*, p. 192.)
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> "The anæsthetic agents, principally chloroform and ether, act directly upon the nervous centres, and have for effect to veil, or temporarily destroy general sensibility and motor power. This action, which might have been *a priori* suspected, has been confined experimentally by the researches of physiologists, and in particular by those of Flourens." * * * "The anæsthesia produced by chloroform and ether determines, in the nervous centre, modifications differing in every point from those produced by carbonic acid or the oxide of carbon." (*Dictionnaire.*)

We shall not enter upon the question whether anæsthetics exert a favourable influence or not upon the result of operations. We supposed it was no longer an open one. The investigations made and statistics gathered in consequence of the rough challenge given to them by Dr. Arnott in 1856, seemed to have satisfactorily settled the point that no unfavourable effects resulted from their use either upon the healing of wounds, the occurrence of accidents or diseases, or upon the mortality after operations. Recently, however, a surgical writer in our own country has re-opened this question. Dr. Hamilton, in his *Treatise on Military Surgery*, not only expresses his doubts upon this point, but is decidedly of opinion that they "produce certain effects upon the system which tend to prevent union by the first intention, and consequently they must be regarded as, indirectly, causes of suppuration, pyæmia, secondary hemorrhage, erysipelas, and hospital gangrene." The charge is a most serious one, and emanating from an authority so high demands the most careful attention. As it can only be sustained or refuted by a further observation of facts and by carefully collected statistics, we refer the decision to the stern tribunal of clinical experience; merely remarking that, as it now stands, all authorities upon anæsthetics, as well as the leading surgical writers of the world, are against our countryman in this matter.

But the great point connected with anæsthetics is, as we have before intimated, the fact that alarming symptoms and even death sometimes occur during their administration. Were there "no danger in what show of death they make," not only would our present task be easy, but the world would possess at least one good unmixed with evil. As it is, the amount of danger, the sources of its origin, and the power of our art to prevent it, as well as the relative agency of the patient, the remedy, and the mode of administration in producing it, are among the most interesting and important questions connected with the subject. We believe that incorrect views in regard to them exert an injurious influence upon the use of these agents, and tend to limit and restrain their administration to the injury of suffering humanity, if not of the profession. Certainly these points deserve our serious consideration however rarely danger may appear and death occur, compared with the vast number of inhalations, and to this portion of the subject we shall devote considerable attention.

First, as to the number of deaths which have occurred from chloroform. Sabarth and Perrin both give "about two hundred," but as the former includes several cases of suicide, and both give deaths which followed several hours consecutively to the administration, and many which are so indefinite as to be unreliable, both lists should be subjected to close examination for scientific purposes. Indeed, Perrin does this and fixes the number of fatal cases due alone to chloroform at seventy-seven. We shall follow Dr. Sansom, the latest authority; he has collected fifty-six cases, which, with fifty given by Snow, makes one hundred and six, and agrees very closely with the report of the Committee on Chloroform to the Royal Medical and Surgical Society, which gives one hundred and nine.

But accuracy in this respect is not necessary; the only condition in which it would be essential would be that of having the total number of administrations of the remedy. Then we could learn what would be very interesting, if not instructive, to know: the relative number of deaths to administrations. This it is impossible to learn, and many efforts have been made to get an approximation by estimate, generally ending in the proportion of 1 to 20,000 to 30,000. Lately, Dr. Richardson has been writing the medical history of England, and has transcribed from the books of the various hospitals visited the number of inhalations of chloroform, and he found but one death to over 17,000 inhalations. Yet all these calculations differ much and assist us but little; in the language of M. Perrin, "they are as easy to produce as difficult to contest," the fruit of "a sterile erudition which dazzles but never enlightens." All we can say is, that some persons have enjoyed complete immunity from accident who have dealt with the agents daily for years, as is the case with Velpeau and Syme; and the instance of the French Army in the Crimea with 30,000 administrations and no death is remarkable.

The period of administration at which death occurred is of the deepest interest, both theoretically and practically. Thus of the 109 deaths a large proportion took place early in the process, before the commencement of the operation for which it was given; "a strange and certainly a noteworthy fact." The table shows within a fraction of fifty per cent.:—

Before full effect of chloroform	50
During " "	52
Not stated	7
	109 (*Sansom*, p. 68.)

In regard to sex, all authorities agree that men are more subject to death than women, notwithstanding the number of administrations in obstetrics. Snow gives the proportion as 3 to 2; Scoutetten, 2 to 1; Kidd, 4 to 1; and Sansom, and the report of the chloroform committee, which we should consider the most reliable, respectively 2.8 to 1, and 72 males to 37 females.

Age has been shown to have a powerful influence upon the mortality under chloroform. Thus, while it has been given at all ages, from a few days, or even hours, up to the extreme limit of old age, the largest proportion of deaths have occurred in middle age, at the period of most robust health and greatest vigor, and children have shown so great an immunity from accident as to cause the anæsthetic process in them to be considered entirely free from danger. Time, alas! has proved this to be a fallacy; even since the publication of the latest work upon this subject this new and important, but sad truth, that children are also liable to accident from chloroform has been learned. While Sansom gives in his table no case of death under five years, a case at two years, which occurred at Liverpool, has been reported,[1] and another at four years is alluded to in a discussion at the Société Impériale de Chirurgie,[2] although we have seen no report of the case.

The condition of the patient exercises an influence widely different from what would be expected, although authorities do not seem to agree upon the matter:—

> "It certainly appears that the healthy and strong stand a worse chance with chloroform than those who have been debilitated by disease. * * This is so supported both by figures and by individual observation, that we can scarcely refuse to receive it, as it has been entitled—the '*law of tolerance.*'" (*Sansom*, p. 67.)

This we believe is the general and correct doctrine, yet M. Giraldes differs from it, and agrees more nearly with Hamilton:—

> "There is meanwhile one pathological condition which demands extreme prudence in the employment of anæsthetics. I allude to those individuals exhausted by profuse hemorrhage, or in a state of pronounced chloro-anæmia, and in whom the nervous system has become very excitable and more susceptible to be impressed by the action of external agents which, like chloroform, affect directly the nervous centres. These conditions, in fact, appear to me to be in some cases a serious objection to the use of these agents, and when we are obliged to make use of them, extreme prudence should be exercised, and the diverse functional modifications which they may produce should be watched with the greatest attention." (*Dictionnaire*, p. 243.)

The kind of operation for which the chloroform has been administered, is, again, the reverse of what would have been anticipated. By far the largest proportion of deaths has occurred in cases of the most trivial operations; a fact which has created universal surprise, and excited deep interest, from the apparent impossibility of furnishing any adequate explanation. We abridge from Sansom (p. 69), the following table:—

Operations of minor import—such as extraction of teeth, etc.	62
Operations of moderate severity—removal of breast, dead bone, etc.	26
Capital operations	7
Reduction of dislocations	6
Delirium tremens and mania	4
Natural labour	2
	117

It is to be very much regretted that Dr. Sansom has not given us the particulars or the authorities of the last class—the two cases of death during natural labour. Nothing, certainly, would have been of greater interest, for it has been maintained and repeatedly stated that no death ever occurred from the administration of chloroform during natural labour. If these are cases such as have been brought forward by those opposed to the use of chloroform in obstetrics when the patient administered it to herself, no one but the nurse being present, we should like to know it. Equally should we like to learn if there have really been two cases of death under these circumstances, and the particulars concerning them. As it is, we have looked in vain for them; Dr. Sansom's book contains only the line we have quoted; the other works not a word. We thought we should find them in the list of deaths of the report of the chloroform committee, but they are not there, and Dr. Sansom quotes from this report, without remark, in a subsequent chapter, that "there is no well-authenticated instance of sudden death recorded, either in this country or abroad, as occurring from the administration of anæsthetics during natural labour, when such administrations have been conducted by a qualified medical man."

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[1] Medical News and Library, January, 1866.

[2] Gazette des Hôpitaux, Juin 23, 1866.

These are some of the most notable facts connected with chloroform accidents which we have thought necessary to briefly mention before proceeding to a statement and examination of the doctrines of our authors in regard to the manner of dying and the probable determining cause of death. That equally able men are not agreed upon this point need excite no surprise, and will not with any one possessing but slight acquaintance with the difficulties which embarrass a decision. In regard to the symptoms when death takes place, the reports of fatal cases are written after the fact, and are the remembrance of events which occurred with great rapidity and under an agitation of feeling on the part of the observers easier imagined than described, and written too with a very natural regard for those most deeply concerned. Experiment upon animals, so valuable a means of determining such questions, fails here because it seems alone to teach us that we cannot produce death in them with the suddenness and rapidity of chloroform death in man. When it is said, finally, that examination of the body after death throws no certain light upon the mode of dying,[1] the great difficulty of the subject will be seen at once. It is, in fact, one of the most abstruse problems ever presented to the human mind for solution.

The first statement to be made in regard to these deaths, is that undoubtedly a considerable number, and especially of those which occurred at the very commencement of the inhalation, were entirely independent of the chloroform. They were purely the result of emotion; a snapping of the vital cords strained to their utmost tension by the arrival of the long-dreaded moment for the operation. Such sudden deaths during, or even just before an operation, have always been known, and many such have been reported. The case of Desault's is well known; he traced with his finger-nail the line of incision in the perineum of a patient bound up for lithotomy, and the man shrieked and died. The case of Simpson's is more note-worthy because chloroform was to have been administered for the first time; the doctor was delayed, and the operation proceeded without it, the patient dying suddenly after the first incision. Had it been given it would doubtless have been not only the first but the last administration! All our authors agree that many of the sudden and early deaths were really caused by psychical influence and not by the anæsthetic. Thus, Dr. Sansom says:—

> "It is reasonable to suppose that fright or apprehension may have had some share in causing the mortality among those who have succumbed before the symptoms of anæsthesia have been induced—that chloroform may have been but one (and that perhaps the least) among the influences." (p. 66.)

We believe this statement far too moderate, and think that few are aware how frequently such deaths occur. The number collected or mentioned in these works has certainly surprised us. M. Perrin, in his introduction, gives the following:—

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[1] "In the present condition of the subject it must, alas, be said, that the diagnosis, the matter of fact of death by chloroform, is to be established more by the circumstances preceding the decease, than by the results of the *post-mortem* examination." (*Saburth*, quoted from *Casper.*)

> "A robust man died suddenly of pain during the efforts made to reduce a suspected luxation of the knee.
>
> "A soldier, aged 25, affected with phymosis which concealed fungous growths, submitted very reluctantly to an operation. No sooner did he see the surgeon's hand armed with a bistoury than he fell into a syncope and instantly died.
>
> "A woman, 45 years old, of bilious temperament and very irritable, had a cancer of the breast. She only consented to an operation with the greatest difficulty. During the operation she died in the presence of the assistants, even under the hands of the surgeon, just as he had made the two incisions and began to dissect out the tumour.
>
> "An old man, who was suspected of having stone in the bladder, was to be sounded; he had an unconquerable fear of this investigation. Scarcely had M. Civiale approached the meatus with the sound, than, seized with terror, he died in the presence, and in spite of the earnest cares of such celebrated practitioners as Civiale and Honoré.
>
> "Similar accidents have happened twice at the Hôtel Dieu, in the practice of Roux.
>
> "This list could be increased, if necessary, by the well-known cases of Garangeot, Chopart, Desault, Cazenave, and several other authors."

Dr. Sansom alludes to some of these and to another which occurred to Mr. Stanley.

Dr. Sabarth delivered a woman with the forceps—"an easy forceps delivery," and left her doing well: within fifteen minutes he was called back to her in great haste, and found she had suddenly died. He truly says that had chloroform been administered, however slightly, it would have received the blame of the death. He also gives the following case, which happened to Cazenave, of Bordeaux, and is alluded to in Perrin's list.

> "A man, 40 years of age, was thrown from his horse and received a broken leg. Amputation appeared necessary. The patient was so depressed by the idea of losing his limb, that although the intention had been to chloroform him, this was abandoned, and it was agreed only to make a show of giving it. A cloth, without any chloroform, was held some distance before his face. Scarcely had he made four inspirations, than suddenly respiration stopped, and the heart ceased to beat. All efforts were vain; he was dead."

Lister gives a similar case which occurred in the practice of Dr. Mackenzie, of Edinburgh.

> "Being called to see a gentleman who had fractured his radius, he had some thought of employing chloroform in examining the arm, but, changing his mind, made the necessary manipulations without it. He then proceeded to leave the house, but had not got down the steps leading from the door when he was called back with the announcement that his patient had suddenly expired."

Giraldes, after examining the danger of chloroform administration, comes to this conclusion, which is important enough to be placed in italics:—

> "*The accidents which have been observed do not much surpass in number the cases of sudden death which have occurred without apparent cause during or soon after operations.*" (*Dictionnaire*, p. 245.)

Whether this statement be true or not, it may well be urged in favour of chloroform that by its use in a part of these cases, death would in all human probability have been prevented, by banishing emotion and destroying pain, and that if we are obliged to grant that it sometimes kills, it sometimes also directly saves.

Farther, from the list of deaths charged to the account of chloroform we would deduct all those where a very severe operation was performed, or one accompanied by much hemorrhage. To attribute the death which follows such an operation as removal of the scapula, or, as we have heard, of ovariotomy of forty minutes' duration, to the chloroform employed, is manifestly unjust. In such cases, chloroform should bear a portion, but only a portion, of the responsibility. It comes, as Giraldes well says, "to add a new factor to the causes of death," but to count, or to weigh such cases in an estimate of the danger of anæsthetics is unfair.

Were we writing solely for the *future* of chloroform, there is another class which we should also eliminate from this list, because experience has taught us the danger of its administration under such circumstances, and it can therefore be avoided. We allude to patients suffering from delirium tremens. No less than nine deaths have taken place in such subjects. Indeed, that the habitual excessive use of alcoholic drinks excites the worst possible influence upon the patient in regard to chloroform inhalation, is the universal testimony of authorities. Experience upon this point has accumulated since Nelaton first called attention to the fact, and it may now be said to be the universal conclusion that anæsthetics should be administered to "hard drinkers" only under circumstances of absolute necessity.

Proceeding with our investigation as to the cause of death and the manner of dying, two subjects intimately connected, and commencing with the best established doctrines, we find that death under chloroform does not take place from deep narcotism, or too prolonged administration; upon this point all our authorities agree, although some say "not from an *overdose*," a very different expression, and one to which we do not assent.

> "The history of surgical anæsthesia, of its accidents and its errors, equally teaches us that the misadventures due to chloroform are not the result of a methodical administration too long continued." (*Perrin*, p. 172.)

That chloroform can kill in this way no one doubts; but if a man commits suicide, or kills himself, as Toynbee has just done, by breathing its vapour *à l'outrance*, or a woman during labour inhales it with no competent person to watch its effects, and dies, these facts should not be held against the safety of the remedy when properly used. That chloroform does *not* generally kill in this way, we have absolute proof; first, the early stage of the process at which death took place in so many of the fatal cases, and, second, the known small quantity which was administered in some of them. Still, Dr. Sansom is of opinion that death was, in a few of the cases, caused by too deep narcotism, and thinks this must be looked upon as one of the causes of death.

The deaths under chloroform cannot be ascribed to disease of the heart. As soon as it became known that the heart was depressed in its action by the anæsthetic vapour it was naturally concluded that a diseased organ would feel the depressing influence more powerfully than a healthy one, and an explanation being a greatly needed want, this theory found a ready acceptance. But Snow taught that the assumption was false, and we have scarcely anything to add from these authors to what he wrote upon this point, except that the presentation of their testimony may add to the authority. All concur that disease of this organ is no contra-indication to the administration of chloroform, except M. Perrin, who believes it predisposes to syncope.

> "Over and over again I have given chloroform to patients affected with incompetency of the valves of the heart, and I have found them very good

subjects indeed for its influence. * * * In persons affected with valvular disease—in those in whom cardiac mischief is the result of acute rheumatism, for example—there is no special danger in the administration of chloroform." (*Sansom*, p. 76.)

We might repeat nearly the same in regard to fatty degeneration. M. Giraldes, the writer in the *Dictionnaire*, makes the statement that Snow considered this a substantial contra-indication. But this is not so; he says: "I have given chloroform in numerous cases without ill effects where the symptoms of this, as well as other affections of the heart, were present in a very marked degree."[1] M. Giraldes says that experience has demonstrated that the deaths have been wrongly imputed to this pathological condition. And—

> "Individuals. considered to be labouring under this organic modification, have been submitted to chloroform inhalation with impunity, and some of those who have succumbed during anæsthesia have not presented the lesion in question."

That special care should be exercised in such cases seems reasonable and is generally inculcated. Dr. Sansom would abstain from it in fatty degeneration:—

> "The general lesson inculcated would seem to be this: that in cases of marked fatty degeneration of the heart, chloroform should not be administered; that in cases wherein a debility of the heart is suspected, unusual care should be exercised to administer a free dilution, so that the heart should not be paralyzed by the sudden shock of an influence which it cannot withstand." (p. 77.)

There are some, however, who go much further than this, and boldly claim that by abolishing pain and preventing its influence in producing irregularity of the heart's action chloroform directly favours the safety of the patient. This was Snow's doctrine, and the report he gives of the action of the heart in a patient whom he observed under lithotrity, both with and without the anæsthetic, is one of the strongest proofs of this position, as it is one of the most interesting observations relating to this part of the subject on record.

Mr. Lister is of this opinion:—

> "It might, perhaps, have been expected *a priori* that chloroform. in the early or exciting stage of its operation, would act upon a diseased heart like mental emotion, and cause irregularity or cessation of its contractions; but this does not seem to be the case. Judging from my own experience, I should say that it tends rather to remove [prevent?] intermission or irregularity of the pulse; and on the whole, I believe that chloroform, by preventing shock and mental effort during the operation and anxiety before it, is in reality a great source of safety in heart disease."

Notwithstanding this gentleman witnessed a death under chloroform and the patient's heart was found to be "extensively affected with fatty degeneration," he nevertheless attributes the death to another cause, which will be considered presently—the shock of the incision; a cause which cannot exist with complete anæsthesia; and as to whether chloroform promoted the fatal result says:—

> "My own impression is, that if it had been pushed to the usual degree, the fatal occurrence would have been averted."

The doctrine that death results from a particularly idiosyncrasy of the patient has had several advocates of ability and high professional standing, and at one time it was quite popular. It was, however, long since found unreliable, and is, we believe, at present, entirely abandoned. As M. Perrin justly says, if the deaths resulted from such a cause their number should be more regularly proportionate to the number of administrations, which is far from being the case. He cites too, as going to show the fallacy of this view, that "in spite of a number of administrations relatively limited, the cases of sudden death in the hands of dentists figure in the proportion of at least 10 to the 100 in the necrological table." But there is stronger proof still, in the fact that in many of the cases of death the patients had previously inhaled chloroform; of the 50 fatal cases of Snow 13 had done so; in one case it had been given six times before. This destroys all foundation for the doctrine. Idiosyncrasy is a particular mode of being or of reacting to the stimulus of external agents peculiar to the individual, and so much a part of him, or of his life, that it is ever with him, does not change from day to day, and ceases only when he ceases to live.

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[1] On Chloroform and other Anæsthetics, London, 1858, p. 54.

This conclusion has been almost universally and very certainly arrived at by authorities on the subject: death under chloroform in the human subject does not always take place in the same manner, or as a consequence of the same pathological causes. And it is a decided advance to arrive at such a conclusion, for there has been a tendency to cling to some particular theory of death as the only and invariable one. Such theories have been founded on but partial views of the phenomena, have led to the use of but partial precautions for the prevention of accidents, and have decidedly interfered with further advances of our knowledge. The accepted doctrine now is that death takes place by syncope or paralysis of the heart, and by asphyxia, authorities being divided as to which is the more frequent and which deserves the greater consideration. The able advocacy of Snow gave great predominance to the doctrine of death by paralysis of the heart, and backed as it is by the unimpeachable evidence of experiment as to the depressing influence of chloroform upon the heart's action, it bids fair to maintain its supremacy. We shall see, however, that views differ as to whether this cardiac paralysis is primary or secondary. With Snow it was primary; blood charged with more than five per cent. of chloroform vapour acted directly on the organ, and his experiment of stopping the heart's action in anæsthetized animals, by blowing chloroform vapour on the exposed organ, is strongly confirmatory of this view. Yet there is strong proof and much reason for the belief that this, at least, is not the only way in which the heart is paralyzed, that the paralysis is often of reflex character, secondary to some peripheric excitation or the result of some profound effect upon the centres of the nervous system.

Let it not be imagined that this question as to the mode of death is one of theoretical interest merely; it has practical bearings of very great weight.

> "During the period in which the doctrine of asphyxia was dominant, and during which we were preoccupied with improvement of the manner of anæsthetizing, the annual mean of sudden deaths was about seven for the space of seven years.
>
> "Since the ideas of syncope have prevailed, and that we have learned to place great value upon the moral state, whether in regard to the appreciation of the indications or to the manner of conducting the anæsthesia, the annual mean has remained fixed at three. And meanwhile the manner of inhalation has not been changed; we employ chloroform oftener and with less hesitation. What is this but to say that we know better how to use it, or, more exactly, that we know better to whom to give it?" (*Perrin*, p. xiii.)

We will give Dr. Sansom's views as to the mode of death first; it will be seen that he introduces a third class, one not mentioned by any other author.

"Death in the human subject may take place by three modes. I. By syncope. II. By asphyxia. III. By necremia.

"The death by syncope may be in one of two forms: (*a*) in consequence of the loss of the irritability of the muscle. * * * This is the common form of death, and is akin to the death from shock. * * * (*b*) tonic spasm. This is not usual, but has occurred; the heart having been found firmly contracted and containing no blood. Here would seem to be a sudden influence on the sympathetic—an irritation, not a palsy.[1]

"The death by asphyxia may be peripheral or central: (*a*) from disorder of the lungs themselves, as in cases of disease wherein a rapid congestion may be superinduced; or (*b*), from suspension of the motor power. * * *

"Death by necremia occupies a position between these more decided forms. Death is due to a faulty condition of the circulating blood, which at one and the same time enfeebles the heart, impairs the activity of the nervous system, and prevents the due performance of the vital changes in the systemic capillaries." (pp. 98, 99.)

As to the relative frequency of these different forms, the author does not express himself definitely; he says sudden cessation of the heart's action is the most frequent of all indications of death. He copies the table of the chloroform committee, which gives 62 deaths by syncope out of the 109; 9 in which pulse and respiration ceased together; and only 2 in which respiration failed before the pulse.

Dr. Anstie's conclusions are arrived at after careful study of the subject and from the observation of one fatal case, and of twenty-one cases in which alarming symptoms occurred in his own practice. He believes death takes place in two ways depending on the rapidity of absorption of the vapour.

"When the impregnation of the blood takes place with *moderate* rapidity, the sympathetic nervous system is the *ultimum moriens*, and death *begins at the lungs.*

"When, on the contrary, the circulation becomes very rapidly charged with a large proportion of chloroform, the narcotic effect may fall with such force upon the sympathetic nerves as to extinguish their vitality at once."

* * * "From the fact that in the immense majority of reported fatal cases the first symptom of danger was confessedly the failure of the pulse and the blanching of the countenance, the conclusion appears strongly indicated, that paralysis of the heart is *the* source of danger in surgical chloroform narcosis." (pp. 382, 383.)

Dr. A. is a believer in the direct action of chloroform as it circulates in the blood as such, both in producing anæsthesia by its action on the nervous centres, and death by its influence upon the sympathetic nerves of the heart.

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[1] The following quotations will show how far one of the highest authorities upon affections of the heart is willing to commit himself upon these points. The italics are our own.

"Now, it is infinitely probable, changes occurring in the ganglia themselves, either dynamic or structural, may be the immediate cause of suspended pulsation of the heart in some cases—but the clinical proof is yet wanting. Still, in certain cases of chloroform inhalation, where death occurs through the heart, *it seems reasonable to believe* the vapour, acting directly on the cardiac ganglia, plays a part in instantaneously arresting their activity." (*Walshe on Diseases of the Heart*, Am. ed., p. 134.)

"Does the heart ever become the subject of true spasm, either clonic or tonic? *A positive answer, founded on clinical evidence, cannot, in all probability, be given to this question.*" (p. 135.)

Dr. Ellis recognizes three sources of danger from chloroform:—

> "1st. From its influence over the heart; 2d. From its effects on the functions of the lungs; and, 3d. From its action as a narcotic poison in the production of coma. The two latter sources of danger may arise either from the vapour of ether, or possibly from that of alcohol, as well as from that of chloroform." (p. 29.)

In M. Perrin we find a strong advocate of death by syncope. He has given a full statement of all the reported cases, and entered at very considerable length into a critical examination of the facts detailed, and of the theories which have been advanced to account for them. His conclusion is that "death is the result of dynamic perturbations, of which syncope is the habitual organic expression." (p. 273.)

> "The mechanism of death, judged by the order of abolition of functions, is always the same, and manifests itself as follows: Sudden arrest of the sounds of the heart; immediately afterwards, exhaustion of the forces, definite cessation of respiration, extinction of all vital manifestations." (p. 352.)

But this syncope, according to him, is not the effect of the anæsthetic, although all anæsthetics predispose to syncope, but an accidental complication, independent of the agent, and having some other cause, "which by its nature is independent of, and should be separated from, the phenomena of anæsthesia."

> "Abstracting the exceptional cases in which it may be supposed that death was the result of an imprudent or excessive administration, the facts demonstrate that sudden death during the anæsthetic process is always the result of an accidental syncope." (p. 405.)
>
> "Viewed as a direct effect of the anæsthetic agent, sudden death during anæsthesia cannot be explained by what we know of the action of these agents; it presents none of the principal characteristics of this action; it rests an eternal enigma, surrendered to the convenient interpretations of occult influences far from satisfactory, and, above all, useless. Viewed as an accident, on the contrary, it becomes a regular effect, normal, the nature of which is revealed by the observation of similar accidents which happen without anæsthetics, and, what is more important, the immediate causes of which are deduced without effort, either from the conditions created by the state of anæsthesia, or from anterior conditions of the patient, or also from the manner in which anæsthesia has been practised." (p. 357.)

But with M. Perrin syncope means far more than it does with us. We have seen that Dr. Sansom makes it synonymous with paralysis of the heart; M. Perrin makes it synonymous with "sideration:"

> "In order to avoid all confusion, we will say that sudden death by sideration and sudden death by syncope, or, if you will, by *syncope foudroyante*, represent but one and the same form of accidents." (p. 406.)

"Sideration" is a term which has become pretty familiar, from its use by French writers in connection with this subject. It is a death, sudden, instantaneous, without material cause; the result of some profound dynamic perturbation of the nervous system; and not, as our author states it in this connection, of the nervous centres merely, but of "the entire nervous system, as well the nerves of the periphery as their central point of emergence."

It will be seen, then, that M. Perrin adheres to the *name* rather than the *thing;* his "syncope" includes many forms of death, having widely different pathological orgin. For instance, he considers one class of deaths as by "syncope of psychical origin;" a class which includes all those which we term death from *emotional causes*, a class which more truly than any other is in accordance with his views as to being of accidental origin and entirely independent of the anæsthetic as a cause. We have already shown how frequently death of this kind takes place. Another class he terms "syncope of organic origin," and subdivides it into those arising from the brain and from the lungs, and explains the fatal result by the influence which both exert over the central organ of the circulation. To the latter, if not to both of these, we shall presently give further attention.

There are two curious positions into which M. Perrin is betrayed by his reasoning, and, by the views he has adopted, deserving a moment's consideration. The first is the acceptance of a peculiar idiosyncrasy in regard to syncope, after having rejected such a thing in reference to chloroform, and the second, that a person peculiarly liable to syncope is not therefore a bad subject for chloroform! Both are exhibited in the following extract:—

> "We have combated the idea of an idiosyncrasy special to chloroform, but are obliged to recognize the existence of a certain predisposition in regard to syncope. There are subjects who have faintings upon almost every occasion, and almost at will. This predisposition may be habitual, permanent; such, from experience, should inspire little fear. We have already said that women, in whom such a disposition appears especially developed, are not more exposed than men to the chloroformic syncope. It may be accidental, and develop itself under the influence of multiform causes, capable of modifying, at any given moment, the vital force of resistance, of creating such a nervous condition that the least emotion, the least shock, provokes a syncope. In this is the real danger." (p. 429.)

The practical effect of M. Perrin's doctrines are far too important to be passed over in silence. From the extracts we have given, and from the work at large they are, briefly stated, that sudden death is a danger inherent to anæsthesia, and by virtue of conditions belonging rather to the *subject* than to the agent or to its mode of administration, that this danger cannot be foreseen, and therefore cannot be guarded against:—

> "The cause of danger, being accidental and inherent to the subject, remains permanent as the subject; there does not exist, and there never will exist, any means by which it can surely be avoided." (p. xiii.)

We do not believe it. Meanwhile we postpone further examination of this point to consider for a moment the practical effect of such doctrines upon the every-day practice of our profession. Is it not to restrain the use of this important class of remedies within very narrow bounds? Is it not, in fact, to limit their use to operations of great severity, and to cases of absolute necessity? Faithful to himself, this is what M. Perrin would do. He acknowledges the difficulty of laying down absolute rules; admits that one person suffers more from a slight, than another from a severe operation; but he banishes it entirely from minor surgery (which local anæsthesia will now most surely do), and also from all such operations as those for ingrown nails, extraction of teeth, etc. He banishes it also from all those operations known as "operations of complaisance," those for the relief of deformities, and from use in all those "researches designed to triumph over a supposed simulation or dissimulation, or to clear up a medico-legal question."

And this is the course all those must follow who believe that there is an inherent danger in chloroform which no sign foreshadows and no care can avoid. As the surgeon should never undertake an operation without candidly laying before his patient the risks he must run, so should the chloroformist say honestly, whenever he administers the anæsthetic, if this doctrine be true, that possibly death will occur during the use of it, and the temporary abolition of consciousness prove an eternal oblivion. We will not dilate upon the influence such a communication would exert on the mind of the patient in favouring a fatal event during the inhalation, or upon the result of the operation for which it was inhaled; nor upon the denial thus made to suffering thousands of a boon which more than any other in the records of science deserves the name of heaven-given.

M. Giraldés, in the *Dictionnaire*, finds Snow's theory of death unsatisfactory, and asks what we shall substitute for it. This is his answer:—

> "We must admit that all the hypotheses advanced for the solution of this difficult problem are open, in divers degrees, to some objections. That which appears to approach nearest to reality, and which at least is founded on rational physiological grounds, is without doubt the theory which regards the mortal accidents during anæsthesia as produced by an apnœa, occasioned by an accumulation of the anæsthetic fluid in the medulla oblongata."

But this theory is open to the objection that it does not account at all for the *early* deaths and especially for those where very little of the agent was inhaled. It is based, of course, on the statement of Lallemand, Perrin, and Duroy, in regard to the amount of chloroform in the various tissues. We are ready to agree to the general statement of this author that it is "by an intervention in the functions of the nervous system that the sudden arrests of respiration, syncope, and sudden deaths are produced," but cannot yield assent to his explanation that these effects are produced by an unusual quantity in the nervous centres. Sometimes this is undoubtedly true, but cannot be in regard to those which occurred at an early period of the inhalation.

We shall not attempt to follow Dr. Sabarth through an examination of the different theories he has collected from various writers, but content ourselves with a table showing the number of deaths by each form, compiled from a careful examination of all the cases he could find, reported with sufficient detail and accuracy for such a purpose; they were only 47 in number, and his conclusion as to the mode of death is from the symptoms which presented themselves and the results of the post-mortem examinations:—

Asphyxia, respiration ceasing first	36
Syncope, pulse ceasing first	11
	47

Mr. Lister declares himself a believer in the doctrine of death from too long-continued administration of the vapour. As between danger from failure of the circulation and from cessation of respiration, he is strongly on the side of the latter. Unfortunately he has adopted decided views in favour of the administration of chloroform from a cloth. He seems bound to prove that Snow was wrong in "assuming that when chloroform is given from a folded cloth, it is apt to be in too concentrated a form," and that most of the deaths that have occurred should be attributed to paralysis of the heart from the vapour being inhaled without proper dilution. He therefore writes too much like a partisan upon this point; worse still, he experiments like a partisan. For instance, he saturates a cloth with chloroform, suspends it before his face, notes the rate of evaporation as shown by a scale-beam to which the cloth is attached, but *inhales through a tube, from a distance*, instead of inspiring the air and vapour immediately about the nose and mouth, and claims that the inspirating current "does not materially affect the rate of evaporation," and that the conditions are similar to those of a patient inhaling the vapour for surgical purposes![1] We think common sense will negative any amount of such experimentation.

These are his conclusions:—

> "The theory of syncope from too great strength of the anæsthetic vapour when the cloth is employed being erroneous, the greater number of the deaths remain unaccounted for; and, if we except a very few instances for which we seem to have nothing to fall back upon but an idiosyncrasy so rare, that it may be practically left out of consideration altogether, their explanation will, I believe, be found in an overdose of this potent narcotic from too long-continued administration." (p. 99.)

Throughout Mr. Lister's remarks upon this branch of the subject, we find the early deaths ignored and the stage of stertorous breathing is looked upon as the only one in which the patient is in peril. To this stage of anæsthesia, or rather to the respiratory process during it, he has paid particular attention; he goes at length into an examination of the mechanism of stertorous respiration, and is a warm advocate of the efficacy of forcible pulling forward of the tongue as a means of "averting danger only." One very important point he insists upon is that stertorous breathing may "become aggravated till it passes into complete obstruction to the entrance of air into the chest, though the respiratory movements of the thoracic walls still continue." All this portion of the article is extremely interesting and of the very highest practical value.

Mr. Lister does not hesitate to carry his doctrines into practice, and boldly follows the path along which his conclusions lead him:—

> "The very prevalent opinion, that the pulse is the most important symptom in the administration of chloroform is certainly a most serious mistake. As a general rule, the safety of the patient will be most promoted by disregarding it altogether, so that the attention may be exclusively devoted to the breathing." * * * "Even when serious disease of the heart is known to exist, it must be remembered that there is much less risk of syncope than of obstruction to the respiration." * * * "Preliminary examination of the chest, often considered indispensable, is quite unnecessary, and more likely to induce the dreaded syncope, by alarming the patient, than to avert it." (p. 105.)

We have now pretty fully presented the prevalent doctrines as to the mode of death under chloroform. If we were restricted to the choice of a single authority upon this point among those we have cited, it would be Dr. Anstie; his views are rational, supported by facts, and, so far as they go, satisfactory. But that there may be other modes of death, appears certain. There has always seemed to be a tendency to restrict the fatal influence of anæsthetics too exclusively to the *chest* organs; this is the great objection to Snow's theory, and to some others. Since the central nervous system has been acknowledged to be liable to a fatal influence from chloroform, whether from too prolonged or too rapid administration, there appears a like tendency to restrict the fatal influence to that part alone, ignoring other parts of this important system, equally important, and, in our opinion, equally influential in producing death.

There is one mode, in which we believe death may be occasioned, which we are surprised to see has not received any attention from writers upon this subject. We allude to death by shock, the fatal impression being a sudden influence upon the branches of the par vagum in the lungs, produced by the inhalation of undiluted vapour of chloroform, this impression being transmitted by reflex action upon the heart. The pathological condition is Sansom's syncope from "loss of the irritability of muscle;" the cause, which he does not give, is, as we have indicated—a profound and sudden impression upon the peripheral extremity of nerves distributed through the lungs. He says this form of death is "*akin* to death from shock;" we say it *is* death from shock. Why not call it so? Certainly it is preferable to the adoption of the French word "sideration," a term much better adapted to concealing our ignorance than expressing our ideas, and far inferior to an English word which conveys a distinct idea to every medical mind, however little we may know of the ultimate pathology of the condition itself.

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[1] See note to his article on pages 97 and 98.

But will reason and the facts sustain this view? Let it be remembered we are advocating the doctrine that it is *one* of the modes of death, not the exclusive mode, but still, we believe, the most frequent one of all.

> "There are two modes of death from chloroform—the first, *and most frequent*, sudden death at the early stage of its influence; the second, a gradual mode, the comatose state, etc." (*Sansom*, p. 72.)

It is to the first class alone the doctrine we have presented pertains. And, first, we say, it is consistent with analogy. That sudden impressions upon the peripheral extremities of nerves produce profound effects upon internal and vital organs is one of the best known facts of physiology. The cold water that, slowly applied to the hands and face, produces no perceptible influence upon a person, when dashed in his face causes immediately spasmodic efforts of respiration—catchings for breath, and consequent irregularity of the action of the heart. If it be objected that the effect is not profound enough to make the cases parallel, we reply that the exciting agency being increased in power, from water to chloroform, or the nerves affected by the agent increased in sensibility as from those of the external to the internal surface of the body, we may look for proportionably greater effect. But there is a more direct answer than this; half a pint of the same agent, water, drank ice-cold in certain states of the system has over and over again caused instantaneous death. Moreover, we say that in such cases death was caused by a profound impression upon the nerves of the stomach reflected upon the heart, and we call it death by shock. Again, the same is true of alcoholic drinks, between the effects of which and of anæsthetics there is so close a parallel; the brandy which, diluted and slowly drank, produces only intoxication, when drank clear and at a draught causes instant death. And we say, too, that this was death from shock.

This theory agrees with the manner of death. It is sudden; the inhalation seems going on well, when all at once the patient starts up or struggles a moment, or suddenly turns pale, and is dead; "like an electric current arrested in its course." Just so death from *emotion* takes place, and death from emotion is death from shock.

It explains the *early deaths*. Can as much be said for any other theory? We believe not. It is not necessary for death to be produced in this manner that the inhalation should have been long continued, nor that much chloroform should have been poured out for administration; the sole condition is that the patient should get a single inhalation of vapour entirely or nearly undiluted with air; the sudden impression on the vast ramification of nerves is made, transmitted, reflected, and death occurs. A moment of inattention on the part of the administrator, a hasty or incautious increase of the amount on the cloth or sponge, and it is done.

It will undoubtedly be advanced here that the air-passages do not bear with impunity very strong chloroform vapour, but coughing is produced, leading to temporary suspension of the inhalation. We admit the fact, but see no difficulty in it. It is only exceptionally, without doubt, that chloroform vapour of sufficient purity to produce the fatal impression can pass the glottis; yet that it *may* do so at any moment is clearly reasonable, for that the watchful guardians of this vital inlet to the body may be *surprised* we all know, or at least we use the term to explain to our satisfaction the fact that a solid body of considerable dimensions, even such a prickly object as a cockle-burr, can be drawn by a sudden inspiration into the larynx or trachea. So, exceptionally, may chloroform vapour of unusual strength gain access, and it is precisely this rare and exceptional occurrence of these early sudden deaths under chloroform which has rendered their explanation difficult.

This theory will explain the deaths which have occurred after previous safe inhalation of the vapour. That sudden death should occur to a patient who seemed from previous repeated inhalations beyond the possibility of meeting with accident was so singular a fact as to attract unusual attention, and numerous explanations were advanced to account for it; none of them were entirely satisfactory, and none of them, we believe, will cover the ground so fully as that of shock.

We have now to examine how such a view of the mode of death agrees with clinical experience. This is the final test of all medical doctrines, and one by which it must stand or fall. Reading over carefully the reports of fatal cases, it is surprising how frequently death occurred immediately after the increase of the amount of chloroform on the cloth or sponge or in the inhaler. We quote from Dr. Sabarth's Case 84;[1] the patient was a boy, the operation for double strabismus.

> "Chloroform was administered upon a piece of lint and produced no unusual symptoms. After three or four minutes the operation was commenced, and, as signs of sensibility manifested themselves, a fresh dose of chloroform was applied. Scarcely had this been done when the countenance of the patient became deadly pale and the pulse, which was just perceptible, disappeared."

This is a type of many of the cases which can be found in the list of chloroform accidents. Looking over M. Perrin's list we find no less than nine which followed very closely this course.[2] It is not necessary to quote them; several can be found in Snow's list. We believe this explanation especially applicable to those cases where there has been a prolonged struggle between the anæsthetizer and the patient, either from timidity on the part of the one or from the latter being of that class of subjects, robust, muscular, and perhaps given to strong drinks—in which the stage of excitement is always longer and more marked than in others. After much time has been fruitlessly expended, patience is exhausted, and a new supply of chloroform is added intended to be sufficient to terminate the contest at once. It does so, but unfortunately in a manner entirely different from what was intended.

But there is also good authority to support this view; if our other positions have all been assailed, we will fall back upon that. Being a question appertaining to the nervous system, we may say that the authority we adduce is of the very higest character:—

"It is by the reflex influence due to the *sudden* irritation of the branches of the par vagum in the lung that chloroform has killed in the very rare cases in which the heart's action has been stopped before the respiration." (BROWN-SEQUARD, *Lectures on the Physiology and Pathology of the Central Nervous System*, p. 160.)

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[1] From Medical Times and Gazette, Oct. 9, 1858.

[2] Cases xi., xii., xvi., xviii., xxvi., xxxv., xl., xlvii., lxvi.

The eminent physiologist is speaking of animals, yet notably, they are more difficult to kill with chloroform than man; so much so, that it is denied by most writers that they can be destroyed at all by the influence of the agent on the heart, and maintained that they always die by failure of respiration from prolonged administration of the vapour. If true of animals, it certainly is true of man.

We do not know that Dr. Brown-Séquard has ever written more fully upon this view of death by chloroform than the few lines quoted above. If he has ever elaborated the above paragraph in any article or work, we have not met with it. Yet brief as is this statement, so consistent is it with reason and with clinical experience, and so high the authority from which it emanates, that we can furnish no explanation of its being entirely ignored by every author who has written upon anæsthesia from the time it appeared in print until the present.

But death may still take place by shock, the initial impression being different—a sudden and profound excitation of the nerves on the cutaneous surface of the body. The able authority we have just quoted gives some striking examples of a sudden suspension of the heart's action from such a cause, and looking over the list of fatal chloroform cases, we find abundant additional evidence. In quite a considerable number of the cases death followed immediately on the first entrance of the surgeon's knife. This was the case in the only instance of death seen by Mr. Lister, already alluded to; the patient started and died as a single sweep of the knife amputated the penis. The author alludes in a note, briefly, to Mr. Bickersteth's observations and opinion, which are given more fully by Snow (p. 240). This gentleman relates three instances in which the pulse suddenly ceased as the first incision was made by the surgeon, and then in a few seconds recovered itself; and he believes that the stoppage of the pulse was the effect of the shock of the incision. Snow dissents from this opinion, and thinks it more probable that the cessation was caused by the profound influence of the chloroform. Nevertheless, Snow's own report of the case of diseased heart, observed by him, where lithotrity was performed with and without chloroform, and which we have already mentioned, is a striking example of the influence of a peripheral excitation, or pain, upon the regularity of cardiac movements.

M. Perrin considers, at length, the influence of the intervention of the surgeon in producing death, and admits its importance, although with him the fatal event is brought about by "syncope." He notices Mr. Bickersteth's observations, and also the investigations made by M. Vigoroux to elucidate this important clinical fact.[1] The conclusions of this gentleman are that the influence of the sensory nerves upon the circulation exists even during the anæsthetic sleep; that it appears to be augmented; that it may be of such degree as to arrest the movements of the heart; and that this was the cause of the majority of deaths from chloroform. Now, as M. Perrin justly remarks, since less than half of the deaths occurred "during the operation," the latter conclusion cannot be true, and he arrives at what we fully believe to be the true solution of this matter; it is only in a certain stage of the anæsthetic process that the sensory nerves can exert this influence upon the circulation; during *incomplete anæsthesia* the heart's action may be thus affected, when it is *complete* the source and origin of the impression, sensibility, is abolished.

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[1] The reference is to "De l'influence de la sensibilité sur la circulation pendant l'anæsthesia chirurgicale."—*Comptes rendus de l'Académie des Sciences*, t. lii. p. 201.

> "Observation in man confirms this view. We have taken the pains to watch the state of the pulse in eight patients during the entire duration of anæsthesia, and we have seen that at the moment of excitation, whether initial or repeated, it marked the reactions indicated by M. Vigoroux, even upon the occasion of slightly painful impressions, the pricking of the suture needle for instance. But, confirmed anæsthesia, as we have defined it, once obtained, the pulse does not betray, in any manner, the *contre-coup* of the operation. The circumstances under which the accidents have happened witness the same thing. Of the eighteen cases of sudden death occurring during the operation, there were ten in which the anæsthesia was manifestly incomplete." (p. 413.)

We think this will answer a question which we could not fail to anticipate, and which, most probably, some of our readers have already put. What is the good of all this minute inquiry as to the exact mode of death by chloroform, and which has so much the appearance of pathological hair-splitting? It is answered, in part, by the latter portion of the above quotation. If there is any possible way of avoiding a death it should be known, understood, and practised upon, and the plain, practical lesson from the above is, that *complete anæsthesia is better than partial.* Better, because safer; a *little* chloroform, so often mentioned, is like a little learning, a "dangerous thing," while full doses carry their own protection from evil.

So in regard to the other channel by which a fatal influence is conveyed to the heart, death occurring from a profound impression suddenly made upon the ramifications of nerves in the lungs. The practical lesson is plain; avoid the danger by a slow and careful administration of the agent. Everything goes to show that it is the *sudden* impression of the agent which causes the shock, and this being understood the danger can be easily avoided. This danger is acknowledged and taught by most of the authors under notice, and it would be manifest injustice not to mention the fact. We have differed from them as to the *modus operandi* of the agent in producing death, but we could not express any more clearly the recognition of danger in sudden interruptions in the inhalation, or warn any more distinctly against them.

> "Rapid and *brusque* inhalations, in which a large quantity of chloroform is given at once, are particularly injurious. First they may poison the patient; but this is the least danger, since it is easily avoided. The greatest objection to them is that they act upon the nervous system by shocks and jerks."
>
> "Experience has demonstrated that among all preventive measures the most important is to proceed with the inhalation very slowly, avoiding all sudden disturbance, all shock to the nervous system." (*Perrin*, pp. 410, 411.)
>
> "In the case of strong atmospheres the danger is due, not only to the quantity of chloroform present, but to the rapid and sudden influence produced." (*Sansom*, p. 83.)
>
> "I am quite sure that the danger of chloroform is not proportionate to the quantity retained in the system—it is its early influence, the 'brusquerie' of its action which has caused the great part of its fatality." (p. 84.)
>
> "Commence the inhalation gradually. Use no force if it can possibly be avoided. * * * Recollect that any sudden increase in the strength of the vapour may produce sudden syncope." (p. 134.)

The latter quotation is from his chapter on the "practical rules for the administration of chloroform," than which we do not know a better chapter in any book of any language; it is a safe guide for the practitioner whatever views may prevail as to the particular mode by which death is caused.

It will have been observed, without doubt, that the tendency of the doctrines we have advanced is to bring the deaths from chloroform to the door of the administrator. We accept the conclusion, for we think it cannot be escaped. Deducting those cases in which emotion played the chief part in producing the fatal event, and those in which the anæsthetic was but one of the "factors," we believe the others were nearly, if not quite, all due to mal-administration. The vapour was given too rapidly, of too great strength, too suddenly, or, in rare instances, for too long a period. The facts sustain this view; the number of deaths from self-administration, the number in the hands of unskilled administrators, as dentists, the manner of death, the circumstances of the administration, the symptoms in cases where death was threatened, but did not take place—all concur in placing the chief responsibility upon the mode of administration and the dose of the remedy. Authorities support this view, and have always done so. It was the essence of Snow's theory that death resulted from too large a percentage of chloroform in the blood, and might always be avoided by restricting the amount. But the fault of his theory was that it took no account of the rapidity of inhalation as bearing on the question of death, and we have seen how very important a part is played by sudden changes of the rate of administration. The authors whose works are under notice generally sustain this view. Sansom says: "It is very certain that want of caution has had its influence in the production of the fatality;" and again "some of the deaths would not have occurred had the chloroform been cautiously and fairly administered."

This doctrine is distinctly inculcated by Mr. Lister. He is trying to account for the deaths having been so much more numerous when the anæsthetic was administered for trivial operations:—

> "When some great operation is to be performed, like the amputation of a thigh or the removal of a stone from the bladder, plenty of well qualified assistants are present, and each of them, including the giver of the chloroform, is duly impressed with the importance of his office, and bestows the requisite pains upon it. But when some trifle is to be done, the whole affair is apt to be regarded too lightly, and the administration of the anæsthetic is perhaps confided to some unsuitable person who also allows his attention to be distracted by other matters."

Dr. Ellis says: "It is, however, only too probable that a still larger proportion of the fatalities laid to the charge of chloroform, might more correctly be ascribed to the mode in which it was given."

There is a table in Dr. Anstie's work from which can be drawn an argument bearing upon this question. Dr. A. has had twenty-one cases in which alarming symptoms occurred out of a total number of 3,058 administrations, or 1 in about every 145! a most surprising proportion, and one which, if generally observed, would, we believe, soon pretty nearly do away with anæsthetics altogether! We felt compelled to try and find an explanation of so singular an occurrence, and one differing so widely from general experience. We think we have found it in the columns which give the dose of the remedy and the period at which the symptoms occurred. From one to three drachms was the quantity; none less than a drachm; too large a dose, we maintain, to be safely commenced with from lint or a handkerchief. In Case 5, ʒj was given on a handkerchief, and the alarming symptoms began in less than half a minute; in Case 8, ʒij were given, and the symptoms began "after a very few deep inhalations;" in Case 11, ʒj on lint, and "after a few inspirations," Case 14, the same exactly; and so on. Is it possible not to connect the alarming symptoms with the mode of administration in these cases? We think not, and we find in this curious table, the like of which we do not believe could be furnished by any other person, several most important practical lessons and strong evidence in favour of the doctrines we have maintained in this article.[1]

It seems to us passing strange that the influence of the mode of administration in occasioning death should have been so generally ignored by writers on chloroform. Reluctance to advance any doctrine likely to cast blame upon members of the profession may have contributed to this, and it would be laudable were blame the necessary consequence, which we cannot admit. But even if it were, it should have no influence in a question of science. Whatever the explanation, the fact remains the same; every explanation seems to have been preferred to this. Now there are three *elements*, so to express it, entering into the process of anæsthesia—the remedy, the patient, and the mode of administration. An impure article of the remedy was maintained as the cause of death as long as it could be; then everything faulty on the part of the second element, the patient, was advanced; idiosyncrasy, diseased heart, and, worse than all—when patients died after having previously inhaled it with safety—the doctrine was brought forward that persons did not always respond alike to its influence! The third element seemed forgotten, yet it is not only likely to be uncertain, but one positively impossible of being uniform under the most usual mode of procedure. The vapour of a liquid is administered whose rate of vaporization differs so much at ordinary temperatures as to be double at one time what it is at another;[2] the liquid is poured by guess on a handkerchief or towel, and held at varying distances before the mouth and nose of a patient, whose rate and depth of respiration vary from moment to moment! And yet, when death occurs, it is assumed, generally, that this process is a uniform unvarying thing, and the deviation from the normal course of events is looked for in the impurity of the remedy or some condition of the patient!

It is chiefly by a more careful administration that we believe children have so generally escaped accidents with chloroform. That they are not absolutely secure we have seen, while numerous theories have been advanced to explain their great proportionate immunity. None of them are entirely satisfactory, while some undoubtedly express a portion of the truth, which is all we will claim for this one. As Mr. Lister maintains a greater bestowal of care upon every step of capital operations, and seeks in that way to account for the greater safety of patients anæsthetized for such a purpose, so we seek in the tenderness of the child-patient, the evident weakness of the trembling form compared with the strength of the giant we hold in our hands, an explanation of its safety under chloroform. The absolute, pressing need of caution and care are forced upon our attention with a power which it is impossible to escape.

It is not consistent with the operations of the human mind that so serious a matter as danger from chloroform should exist without various and persistent attempts being made to remedy it. Different propositions have been made for this purpose from time to time. The first we examine is the substitution of ether for chloroform. That the stronger agent acts more promptly, more pleasantly, and that with it anæsthesia is more easily as well as more rapidly produced, is not denied by the advocates of the weaker, and the sole question to be decided is as to the relative amount of danger in the two. It is claimed by the advocates of ether, by some of them at least, if not by all, that it is essentially harmless—that no well-authenticated case is on record in which death can fairly be attributed to this agent. This claim cannot be allowed upon the testimony of the authors before us. Dr. Sansom alludes to nineteen cases of death from ether, as collected and published by Trousseau; the details are wanting, yet the high authority for the statement will generally be considered a sufficient voucher for its truth and justice. Dr. Sabarth quotes the second of the conclusions of the Boston Society for Medical Improvement—("It is a generally accepted fact that ether is less dangerous than chloroform")—and adds the following paragraph:—

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[1] This table is quoted by Sansom, but the amount of chloroform given is omitted —a most important point.

[2] "Air at 40° Fahr. can retain 6 per cent. of chloroform vapour; at 60°, 12 per cent." (*Sansom*, p. 22.)

> "Thirty-six cases of death from ether are known to have been collected. so that in proportion to the frequency of the administrations of chloroform, and of the much less frequent use of ether, the number is a very large one." (p. 154.)

He believes that chloroform will remain the principal anæsthetic.

M. Giraldes, in the *Dictionnaire*, examines this question at considerable length. He decides, *a priori*, that the inhalation of ether cannot be absolutely free from danger, and that in claiming that it is so the medical societies of Boston and of Lyons have advanced a proposition "much exaggerated." He finds this decision supported by the facts; he examines the cases given by the Boston Society, and says: "This collection of facts, to the number of twelve, show well that the ether has been one of the factors, if not the principal factor, which has occasioned the death;" and thinks the "American surgeons, devoted partisans of ether, carry their enthusiasm too far." Upon the persistent attempts which have been made to explain away the agency of the ether in bringing about the fatal result, he makes an excellent point:—

> "We are obliged to remark, that if in examining the accidents attributed to chloroform, the same vigorous method had been pursued as has been followed with those attributed to ether, assuredly we should have arrived at a diminution of the number of the mortal accidents which cast a shadow over its history."

M. Perrin gives the details of three cases of death from ether. Following the same rules of examination which we should do in regard to chloroform, we reject two of them as not fair cases. In one, at Lyons, the patient was very feeble and prematurely aged, and was advised not to inhale the ether; in the other, at Bellevue Hospital, under Dr. Alonzo Clark, a post-mortem examination revealed a tumour in the right lobe of the cerebrum. The third case we consider an undoubted case of death from ether.[1]

There is another clear case of death from ether on record in this country. We allude to the case reported by Dr. W. H. Mussey, of Cincinnati.[2] If the agency of the anæsthetic has been successfully explained away in this case, we have not met with it. Dr. Sabarth copies it into his work without expressing any doubts.

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[1] The references in M. Perrin's work are carefully given, with the exception of this case! It is not stated where it occurred, or under whose care! The reference is to *Journal Connaissances Méd. et Chir.*, 1842—the date evidently a typographical error. The patient was a Bavarian, it was in an hospital, and Charrière's instrument for the ether was used.

[2] Cincinnati Lancet and Observer, January, 1861.

The relative danger of the two agents then is a point difficult to determine, impossible without figures, which cannot be obtained. Our belief is, that did we know the total number of inhalations of chloroform with 200 deaths, and the total number of etherizations, allowing only the two undoubted cases of death mentioned, the ratio of deaths in the latter would be fully equally to that of the former. The general verdict of authorities is that ether is less likely to occasion death than chloroform, because a weaker agent, and, therefore, better for careless or inexperienced administrators. In our own opinion it is impossible to occasion death by shock with ether; not only a possible but probably a frequent mode of death by chloroform. In this respect the two agents differ just as wine differs from ardent spirits. Enough of the former could not be drank off at once, or would not produce a sufficiently profound impression upon the nerves of the stomach to produce the instantaneous death sometimes caused by the latter. But so far as utility and the demands of practice are concerned, the question has been emphatically decided by the profession—it is pretty nearly Boston and Lyons against the civilized world. Nor do any of the reconsiderations which are from time to time urged upon the profession affect the decision: "As the matter now stands, the use of chloroform is increasing, that of ether diminishing." (*Sansom.*)

The doctrine of Snow that death was alone caused by breathing air too highly charged with chloroform vapour leads directly to the use of an inhaler, an instrument which would always regulate the amount of vapour administered, as a preventive of accident; and since his time there have now and then appeared advocates of inhalers, who would fain make the profession believe such an apparatus to be indispensable, that his own instrument is the best, and by the use of which all danger can be avoided. So little effect, however, has been produced upon the profession, in this country at least, that inhalers might be dismissed without a moment's consideration. As Dr. Sansom, however, again appears as the advocate of an apparatus, and as it appertains to the subject, we give it a passing notice. Among the authors under review we find Sansom, Ellis, and Anstie in favour of the use of inhalers, and Lister, Sabarth, Giraldes, and Perrin opposed to them. The objects to be obtained by them is the insurance of an atmosphere of uniform and moderate strength, not containing above a certain percentage of vapour, Dr. Anstie gives 3.5 per cent., and thinks such an instrument would insure the perfectly safe administration of chloroform. Dr. Sansom has invented a special inhaler, constructed with two principles in view—a definite dilution of the vapour and a gradually increasing strength of it, and thinks he has improved upon Snow's, although in regard to Mr. Clover's instrument he expresses himself as considering it "the safest method of all for the administration of chloroform." Mr. Clover's plan is to use a bag large enough to contain the air for the respiration of several patients during inhalation; into this is introduced sufficient chloroform to give accurately a strength of 4.5 per cent., and, by means of a valve near the mouth-piece, even less than this is administered at first. The bag is attached to the coat or around the neck of the person giving the anæsthetic, and is, of course, costly, cumbrous, and formidable in appearance to timid patients. In Dr. Anstie's opinion, too, this is the best apparatus, although Sansom's inhaler had not been presented when his book was published.

The argument in favour of inhalers made by the chloroform committee, based upon the fact "that not less than 80 per cent. of the fatal accidents with chloroform have arisen when it has been given on a handkerchief, lint, or sponge," we do not consider of any weight, because the number of administrations without an instrument so vastly exceed those with one. Any argument in favour of them, based upon economy in the amount of the anæsthetics used, is unworthy of a moment's consideration. None can certainly be founded upon their superior convenience, for all of them require special study and care in their management. This is especially the case with Dr. Ellis's, an ingeniously contrived instrument for insuring definite amounts of vapour from several fluids of different rates of vaporization, probably well effecting the end in view, but complicated, according to his own testimony, requiring study, and, we believe, utterly impracticable in daily life. It will not do, especially for Dr. Sansom, to base any argument upon the number of times any one of them has been used without accident, for we should turn against him his own reply to those who claim special immunity from accident on account of having followed some special mode: "If you had given chloroform ten, or even twenty thousand times, and had then met with a case of death, you would but have received your just proportion according to recorded cases." The only satisfactory proof would be absolute immunity from accident when an inhaler was used; unfortunately this cannot be furnished. In Dr. Anstie's table of 21 cases in which alarming symptoms occurred, Snow's inhaler was used twice, alone. Another important fact is learned from this table, as well as elsewhere; in one case Weiss's inhaler was used, but was "out of order!" That the mechanism of tubes and valves may become deranged, and thus introduce a new element of danger into the process, is an important fact, and a most serious objection to all these instruments.

> "The employment of inhalers is not only an embarrassment in practice, but we will go further, and say that it introduces a cause of danger." (*Giraldes.*)
>
> "Inhalers have also their sombre list. * * * In the sixty cases in which the manner of administration is indicated an inhaler was used twelve times. * * * Thus in one-fifth of the fatal cases a mechanical apparatus was employed, a proportion which is very considerable when we take into account the limited use of them. * * * It suffices for us to draw this conclusion, the immunity promised in their name remains thus far a Utopian idea. It is a philanthropic dream, but it is only a dream." (*Perrin*, p. 461.)

If we needed any more authority, we should find it in the practice of the most eminent surgeons of the world; those who use chloroform the most frequently, administer it in the most simple manner. Velpeau, Larrey, Cloquet, Nelaton, Caseaux, Jobert, Huguier, Simpson, Syme, Druitt, Erichsen, all reject inhalers. We will not say that there is no such thing used in this country, but we certainly do not know, and have never heard of a surgeon in America who uses them, nor of a respectable authority for their use.

But, in spite of all this testimony, if the question were simply between an inhaler and the most frequent mode of administering chloroform, upon a handkerchief, thick towel, or some article nearly impervious to air, we should prefer the former, believing the latter to be the worst possible plan. There are elements of safety in inhalers; they secure definite dilution of the vapour, and prevent those sudden irregularities and shocks with the vapour, which play so important a part in the production of danger; hence, they are certainly safer in unskilled hands. With the exception of Mr. Lister, the testimony is unanimous as to the evils of administration from a handkerchief:—

> "The great objection to the administration of chloroform on a handkerchief is its irregularity—the utter ignorance we have of the strength of the vapour

inhaled at a given time. At one moment a quantity of air strongly impregnated with chloroform may enter the lungs; at another, a breath may be taken of almost pure atmospheric air." (*Sansom*, p. 79.)

This we know from personal trial; a mere change in the position of the handkerchief may give a whiff of the vapour which is overpowering. Add to this the widely different rate of vaporization of chloroform at varying temperatures of the atmosphere, and the great irregularity of this mode of administration is manifest. The article upon which the chloroform is sprinkled should be porous, and sufficiently so to admit of free respiration through it, and which will insure the entrance of the vapour to the lungs well mixed with air, and not in *strata*. Nothing meets the necessities of the case better than sponge; not a flat or round piece, upon which the liquid is poured and held under the nose, but a cup-shaped piece, fitting over the respiratory orifices. Such a piece, well squeezed out of water, a few drops of chloroform sprinkled on it to begin with, and pressed gently so as to carry the anæsthetic to all the interstices through which the air will pass, affords the best possible instrument for everyday practice. If any one, accustomed to the use of a napkin or handkerchief, will try this plan, we feel confident he will be surprised at the ease and regularity of the production of anæsthesia, and the small quantity of the liquid used.

The use of a mixture of anæsthetic vapours as a means of escaping the dangers of chloroform, is the proposition at present attracting the most attention, and one, we believe, which deserves the most careful consideration. Such mixtures have always been more or less used, but the Report of the Chloroform Committee of the Medico-Chirurgical Society of London first distinctly and unequivocally declared their superiority, and placed upon their use the seal of authority. This was so recent that it is only in the latest works of our list—those of Drs. Sansom and Ellis—that we find any adequate mention of the subject. M. Perrin gives an account of the only death known to have taken place under a mixture of ether and chloroform. The case occurred in this country, and was reported in this journal for July, 1857; the patient was a child five years old, the operation removal of a tumour, and four ounces of blood were lost during the operation. That the principles of administration were not as well known then as now is very certain, and may be fairly urged in reference to this case, even were there no unfavourable "factors" present, a severe operation and loss of considerable blood for a patient of that age.[1]

The object of anæsthetic mixtures is the avoidance of danger, especially of that danger arising from the depressing influence upon the heart's action which chloroform most certainly exerts, which ether does not exert, although the evidence is insufficient to convince us that it stimulates the heart, as alcohol certainly does. The committee proposed three mixtures:—

A.	Alcohol	1 part.
	Chloroform	2 parts.
	Ether	3 parts.
B.	Chloroform	1 part.
	Ether	4 parts.
C.	Chloroform	1 part.
	Ether	2 parts.

The introduction of alcohol is a novel feature in one of these, and Dr. Sansom gives the credit of originating this mixture to Dr. Harley. There is no doubt that the alcohol plays a very important part in addition to and independent of its stimulating effects, either by modifying the influence of the others, or in affecting their vaporization, or both. The committee says it is by "the uniform blending of the ether and chloroform when combined with alcohol, and probably the more equable escape of the constituents in vapour." Dr. Ellis says "the chloroform is, after all, the potential agent; the others are chiefly adjuvants;" * * * "vehicles and diluents of the chloroform." Dr. Sansom[1] gives strong testimony as to the stimulating effects of this ingredient, counteracting the depressing influence of chloroform:—

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[1] Snow does not hesitate to say that the patient died of hemorrhage.

> "In my own experiments I have found that alcohol has had the greatest effect in sustaining the heart's action during the influence of chloroform. I can particularly recollect one instance in which alcohol, having been administered in vapour to a frog, it was impossible to cause death by any strength of chloroform vapour. I certainly think that a dilution of chloroform with alcohol is advisable in many instances, and I am quite sure that the administration by the stomach of a little alcoholic stimulant acts beneficially." (p. 140.)

Dr. Ellis has made the administration of mixed vapours his special study, and is their especial advocate. He believes that anæsthesia by this method "has been proved to be the safest, the pleasantest, and the best, in its total results." A large part of his small book is taken up with experiments upon the differing rate of vaporization of the fluids and the means he has tried to equalize it. In this he claims to have succeeded as well as in devising an apparatus which insures the administration of a larger than usual amount of the vapour of alcohol. In everything relating to these parts of the subject his work is very complete, and his reasoning is good.

The objections urged to anæsthetic mixtures are: 1. The length of time required for the process; 2. The probability of sensibility not being abolished (Sansom); and, 3. The unequal rate of vaporization of the fluids (Ellis). The first two cannot, in our opinion, be sustained. The third "introduces a new element of danger." It was first advanced by Snow. He says when ether is combined with chloroform "the result is a combination of the undesirable qualities of both agents, without any compensating advantage," and that there is great danger because the operator, towards the end of the process, may be giving pure chloroform when he thinks he is giving the weaker mixture of vapours. So positive a judgment by such eminent authority postponed the use of mixed anæsthetics in our hands for many years. Still, the facts of unequal evaporation, and consequent danger remain the same. Dr. Ellis says:—

> "Out of the six or seven minutes occupied by the evaporation of the half drachm of fluid, the first was occupied chiefly by the ether, the next three by the chloroform, with a little alcohol, and the last by the alcohol alone. In an inhaler, the patient would have breathed, for one-fifth of the time, chiefly the vapour of ether; for the next three-fifths, that of chloroform with a little alcohol; and at last only the vapour of a minute quantity of alcohol." (pp. 24, 25.)

We fully recognize the danger, then. But it is precisely one of the best of dangers; if we are allowed such an expression. Best, because being known it is easily avoided; the only precaution necessary is for the operator to squeeze out his sponge napkin or lint from time to time during the administration, and make a fresh application from the bottle.

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[1] Dr. Sansom has lately advocated, before the Obstetrical Society of London, a mixture of equal measures of chloroform and absolute alcohol, as "an excellent anæsthetic, which gives off a proportion of chloroform vapour in a given time, almost exactly half of that which is given off by chloroform pure and simple." It is this mixture, we understand, which was employed some years since in the Massachusetts General Hospital under the name of chloric ether.

Our own experience with mixed vapours, and we have used only mixture A, has been exceedingly satisfactory. Our experience has not been great, but we have never been much longer in producing anæsthesia and have never failed of making it perfect. One case which fell under our observation afforded an opportunity of contrasting the effect of the mixture with that of undiluted chloroform and presented a striking instance of its superiority: A young man, muscular and robust, from appearance probably a moderate drinker, presented himself at our office with dislocation of the shoulder of five weeks' standing. Before attempting reduction he was placed under chloroform, and the symptoms were by far more unpleasant than we had ever before or since met with. The struggling was great, the suffusion of countenance extreme, the respiration irregular and catching, and the veins of the neck and face turgid with blood; it was only with the greatest caution that we carried him to the stage of complete relaxation. The attempt at reduction failed, and in an hour or so he consented to a second attempt. This time we resorted to the mixture, and the contrast was striking; he went under its influence with no more than ordinary difficulty, if as much. The influence of the previous administration of chloroform in changing the patient's condition—the only "element" in the case which could be modified—we cannot estimate very highly, and therefore look upon it as a fair instance of the comparative effects of the mixture, and pure chloroform.

In our opinion anæsthesia by mixed vapours is destined to increase in use and in favour with the profession. The method seems especially adapted to cases in which the operation is likely to be of some duration, or accompanied with much hemorrhage, or other depressing circumstances, and those who believe in inherent and unavoidable danger in chloroform should by all means resort to it.

But whatever anæsthetic may be chosen, ether or chloroform, the pure article or the mixtures, and whatever instrument or mode of administration may be preferred, nothing will obviate the necessity of *care* in the administration of these powerful agents. This is at once the teaching of reason, of experience, and of the best authority. They have been too long and too frequently intrusted to unskilled hands, a dresser or student, and the wonder is that death has not oftener occurred. Happily, this custom is passing away, and the necessity for careful study of the subject is fully recognized.

> "We believe we shall render a veritable service if we vulgarize this idea that anæsthesia should be observed and studied at the hospital with as much care as every other subject of practical medicine." (*Perrin*, p. 417.)
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> "The administrator should be experienced. It has been in some cases a custom to intrust the chloroform to a young dresser—one who might be more anxious to watch the progress of the operation than the stage of the narcosis. This is a state of things to be deprecated, and several hospital committees have acted wisely in appointing a Chloroformist—a measure which is not of less value to the operating surgeon than it is of benefit to the patient. One who administers chloroform in any case should confine himself exclusively to the task he has undertaken, and should constantly mark the symptoms." (*Sansom*, p. 131.)

There is no one point attention to which alone constitutes care in administering anæsthetics There are three indices of danger—the pulse, the respiration, and the countenance; attention to all of them is necessary, for a change in one or the other has always marked the appearance of alarming symptoms.

> "The administrator should observe the countenance, the respiration, and the pulse. There has been too much dispute as to which should receive most attention. Some writers have urged attention to the pulse to the exclusion of the breathing, others *vice versa*. The truth is, to my mind, that all the signs should receive attention—none more than the others." (*Sansom*, p. 134.)

The only exception to the inculcation of such doctrines is Mr. Lister. We have already seen that he neglects attention to the pulse, and we regret to find that he fails to impress that caution which we believe to be so necessary, and that his language is such as to allow the student to infer that, after all, anæsthesia is a trifling process, requiring neither special study nor experience.

With this exception we know of no authority which does not urge in the strongest terms the necessity for caution in the administration of these agents. Even M. Sédillot, who promulgated early in the history of anæsthesia, and ever since, the doctrine that "chloroform, pure and well administered, *never kills*," is careful to teach with it that anæsthesia "is an art which requires attention during every moment, much skill and experience," because "every time chloroform is administered life is at stake."[1]

Drawing our work to a conclusion and surveying carefully the ground we have passed over, we ask, what is really the amount of danger? how high shall we estimate liability to accident, the one drawback and objection to anæsthetics? It cannot be given in figures, but must be estimated, and find expression in general statements. We draw the conclusion that it is very small indeed from Mr. Lister's article alone. He pursues a mode of administration which we believe to be faulty in the extreme, does not teach very strongly the necessity of special study or experience, has evidently seen a vast number of administrations, and yet has no fear whatever of the agent. He says "it may be used so as to be practically free from any risk whatever;" and we think he furnishes himself the strongest illustration of the fact!

Dr. Anstie does not place the estimate higher, although he has witnessed one death from it, and met with alarming symptoms far more frequently than falls to the lot of most men. He does not hesitate to counsel its use upon every occasion. In the following extract the italics are his own:—

> "It is my firm persuasion that, with proper care, chloroform may be safely administered *to any patient who is fit to undergo an operation at all, whether there be any existing disease of heart, lungs, or brain, or not.* I have never allowed the known existence of such disease to prevent my administering it, and I have never found any evil result. I entirely concur with Dr. Snow's opinion on this subject." (p. 384.)

Dr. Ellis, a special pleader for the superior safety of mixed vapours, speaking of the danger from chloroform, says:—

> "The risk is, of course, small; all statistics go to exhibit that important and reassuring fact." (p. 1.)

We translate from Dr. Sabarth's preface:—

> "From a statistical point of view it must be confessed without restriction, that the evil consequences of this wonderful agent, whether of temporary duration or fatal nature, appear minute when compared with the innumerable multitude of fortunate results which almost every country of the civilized world can show, and can scarcely be considered of any weight in the decision of the question."

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[1] See an article in Cincinnati Journal of Medicine, Aug. 1866, translated from *Gaz. des Hôpitaux*; and his *Médecine opératoire*.

M. Perrin, who believes, as we have seen, that there is and always will be danger in chloroform inhalation, and that our chief advance will be by learning better to whom to give it, speaks hopefully of the future:—

> "We are making progress; we have made it in the past and shall continue to do so in the future, and the mind can foresee the time when the skill of the administrator will enable him to escape every evil predisposition." (p. xiii.)

Sansom says:—

> "I believe that the facts warrant the conclusion that the dangers of chloroform have been exaggerated." (p. 10.)
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> "From all the considerations I have advanced, I think the following conclusions may be drawn: That though chloroform has in many instances caused death, it has not the fatal power which might at first sight appear. That, excluding the various chances which all chloroformed and not chloroformed must submit to, by utilizing our increased knowledge, and exercising an increased care, we may deprive it of all its terrors." (p. 17.)

The amount of danger is not only small but, more important far, it is in a great degree preventable. Probably we shall continue to have deaths from emotional causes so long as the human mind continues what it is; so long as the body remains what it is we shall have those deaths in prolonged operations where chloroform is "one of the factors" in producing it. But with "our increased knowledge, and exercising an increased care," we look for the others to disappear from the annals of medicine. Nothing will tend so powerfully to promote this result as a careful study of the chloroform literature of the present. Not only will such a study increase the frequency of its use until the axiom of Syme is generally accepted, that "every case for operation is a case for chloroform," but it directly and powerfully adds to the safety of the patient and the comfort of the operator. No contrast can be greater in either respect than that between an administrator of anæsthetics, who is timid, and justly so, because he is a believer in the pernicious doctrine that no reliance can be placed in the regularity of their action, and one who is imbued with the doctrines that irregularities of action arise from irregularities of administration, who has confidence in his remedy and feels that he has the power to control it. The one dreads the arrival of the hour for the inhalation, and thanks his stars when it is safely over; he is fearful, and administers the vapour irregularly; while the patient, escaping the dangers of inhalation by jerks, is exposed to those arising from incomplete anæsthesia, and the surgeon is annoyed and baffled in his manœuvres by the struggles of partial sensibility. The other proceeds steadily but cautiously to the end in view; he "makes haste slowly," and with a boldness tempered by wisdom carries his patient down into the dark valley which borders on death, drowns human agonies in the waters of Lethe, and triumphs in the crowning glory of his art.

If the present of anæsthesia is cheerful and satisfactory, the future, too, looks bright and promising, and we cannot close without calling attention to the fact. The number of agents known to produce anæsthesia, the extent to which they are being tried, and the amount of attention directed to improving this branch of our art, all give evidence of farther advances in our knowledge. We think no one can rise from a careful study of the present condition of the subject without feeling convinced that we shall not much longer be limited to the agents now known, and that the day is not distant when, to quote the eloquent address of Mr. Bowman to the British Medical Association, "in the sure progress of our art, some other anæsthetic shall be found without even the slight inconveniences of chloroform."

J. C. R.